case nor in the other can the equation (3) be made to prove the quoted statement with which this article begins.

Although the above considerations are theoretically of profound importance, they will obviously have no influence whatever on astronomical calculations; for in all such calculations the product of mass into Newtonian constant is used. Suppose in the case of the sun $G_S > G_M$ and let S, S′ be the true sun's mass and its present accepted value, then

$$G_S . S = G_M . S'.$$

In no calculation do we require S alone, but always product $G_S . S$, so that the false values for the sun of G_S and S do not lead to error.

P. E. Shaw.

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The Spectrum of X-Rays.

(1) Kossel (*Verh. d. D. Phys. Ges.*, 1914), discussing the previous measurements of the frequencies of the corresponding lines of the spectrum K and L characteristic for different elements, as well as of the limits of the respective bands of absorption, established certain numerical relations among them which Wagner (*Ann. der Physik*, 1915) confirmed in a more perfect way and even generalised. In addition, the extremely precise measures of Bragg and Pierce (*Phil. Mag.*, October, 1914) for the lines K_α, K_β, K_γ in the Ag, the Pd, and the Rh give additional confirmation.

From among these relations, attention is specially directed to the one obtained by generalising the second law of Kossel—

$$L_\alpha = K_\beta - K_\alpha, \text{ viz., } \quad (1)\ M_\alpha = L_\gamma - L_\alpha$$

In these equations each term expresses the frequency of the lines. With its help we can foresee the frequencies of the principal lines of the spectrum of the M characteristic if L_γ and L_α are known.

If we admit Bohr's model for the atom as modified by Kossel—that is to say, supposing that in the transit of an electron from one ring to another a quantum is emitted, the frequency of which is determined by Bohr's formula, the radiation M will correspond to the transit of an electron from the fourth to the third ring, and the corresponding frequency,

$$\nu = \nu_0 \left(\frac{1}{3^2} - \frac{1}{4^2} \right) \left(N - a \right)^2 = 1.59 \times 10^{14} (N - a)^2$$

will be emitted.

To demonstrate the exactness of this prediction, I have calculated the values of ν contained in the following table by means of the formula (1), starting from the values of L_α and L_γ given by Moseley, Rutherford, and Andrade. The last row contains the values of ν obtained from the formula—

$$(2)\ \nu = 1.39 \times 10^{14} (N - 21.8)^2$$

deduced by graphic interpolation, where N is the atomic number of the element.

	N	ν cal. by (1)	ν cal. by (2)
Pd	46	81×10^{15}	82×10^{15}
Ce	58	189	182
Eu	63	245	236
Ta	73	364	365
Pt	78	438	440
Au	79	452	455
Radium B	82	501	505

As can be seen, the formula (2), of similar type to those found by Moseley for K_α and L_α, shows well enough the change of M_α with N, and besides the constant $1{\cdot}39 \times 10^{14}$ coincides with the theoretical value $1{\cdot}59 \times 10^{14}$, between the limits to be expected on account of the method of calculation. No data are at present available to test this hypothesis. The spectrum M is most likely to be observed in heavy elements and in the radio-active bodies.

(2) In the numerical relations obtained by Kossel and Wagner, there does not appear the groups L_{A_2}, L_β, and L_δ of the spectrum L, nor does there seem any simple way of introducing them in another series of formula involving the frequency of the K radiations. This suggests the possibility of the spectrum being composed of two distinct parts, which might be illustrated by supposing that two rings are situated in distinct planes, from one of which (the one corresponding to L_2) no electron can pass to the ring K. The correctness of this hypothesis could be tested by investigating experimentally the lines of the L spectrum that accompany the spectrum K—a study that does not seem impossible.

It is logical to suppose that this group L_2 should be accompanied by others, M_2, N_2, . . . in the exterior spectrum, and even that in these should appear some new ones—M_3, N_3 . . . N_4 . . . In this way it would be easy to understand how the spectrum gets more and more complicated from the X-ray spectrum to the visible, and even the existence of the well-known series in the light spectrum.

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Studies of the Cotton Plant.

I should be sorry if any reader of Nature were to be prejudiced against perusal of my book on applied plant physiology, called "The Development and Properties of Raw Cotton," by thinking that I had attempted to write on the systematic botany of Gossypium. Yet this impression might easily obtain, since your reviewer devotes exactly two-thirds of his review to a few pages of my first chapter, the title of which, "The Development of Pedigree," is alone sufficient to indicate that it was not intended for biologists.

Still, technological treatment is no excuse for inaccuracy, even as enthusiasm for herbaria is no excuse for such a phrase as "peripatetic Mendelian cross-breeding of undetermined stocks." Therefore I would direct your reviewer's attention to the fact that my description of Gossypiæ as a sub-order (instead of a tribe) is taken from an accepted authority on systematy.

He states that "the results and evidences of systematic botany" with regard to cotton have been before us for a long while; the Hindi weed-cotton to which he purposely refers is a sad example of the result. I regret being obliged to mention it, but he misspells it as "Hindu" weed, and then objects strongly to my leaving it with such a loose designation. Now, in the first place, this name is on record in Sir George Watt's standard monograph of the genus, and is therefore as definite as any other. Possibly more definite, for though the specimen used for that determination came from a pure strain, Sir G. Watt called it "a ferine hybrid possessed of a strain of *G. vitifolium*." Only by enlisting the aid of "peripatetic" Mendelism can one justify the apparent contradiction, but nothing *a priori* can excuse the inclusion of this Hindi weed in the "fuzzy-seeded cottons" during the primary division of the genus in that monograph. There is no cotton which has a more naked seed than Hindi.

It is clear that I took too much upon myself in attempting to persuade the growers and spinners of

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cotton—in the first dozen pages of the book—that the systematic botany of cotton had some definite meaning. Nor did I think that any reviewer would be so ruthless as to drag my little jest (about scientific names appearing to be "merely useless duplicates of easier names") out of its context to pelt me.

W. LAWRENCE BALLS.

Little Shelford, Cambridge, September 16.

MR. LAWRENCE BALLS's objections to my review of his book, "The Development and Properties of Raw Cotton," which appeared in NATURE of August 26, call for a reply from me.

I feel quite sure Mr. Balls need have no fear that my remarks will be viewed, by even the most casual reader, as the criticisms of a work that had attempted to deal with the systematic botany of Gossypium. But Mr. Balls's anxiety that that great sin should not be attributed to him, exposes himself to the charge of deliberate disregard for both the methods and results of the systematist. It is a fact that I specially devoted a considerable part of my remarks to what I regard as the weak side of Mr. Balls's book, and I repeat it is a very weak side, which, though contained in one chapter mainly, dominates his entire studies of the cotton plant. But with equal deliberation, however, I recognised and even extolled the meritorious features of the book, which are undoubtedly very great.

The implication that I read only certain portions of Mr. Balls's book is quite uncalled for. As a matter of fact, I read every word. It was only because I appreciated and even admired the book that I felt it incumbent to express my mind unhesitatingly. It was in no spirit of carping that I gave special attention to its shortcomings. The issue at stake is very great indeed; namely, the development of the cotton staple, a problem of Imperial interest in the agriculture and industry of our Empire. I cannot help repeating, therefore, that for Mr. Balls to attempt to justify Mendelian cross-breeeding of undetermined stocks (and even pedigree selection of such stocks) of Gossypium is not only a blemish but a serious blunder, both in his book and his work. With culture experiments accuracy, in the starting point (more especially with stocks that of necessity involve several species and numerous varieties and races), is more essential than even care in subsequent treatment. We have heard far too much of the assumption that successful stocks can be produced in the laboratory or the experimental plot, in utter disregard of systematic botany.

The sneer that has been thereby cast on herbaria work is uncalled for, and merits the severest condemnation. Such an attitude may enlist the sympathy of the ignorant, but can secure no advancement in the object in view. The question of the future supply of cotton to the British looms is too serious a matter to justify any half-measures. The history of cotton is full of fads and fancies. Extravagant and wasteful experiments have taken the place of rational development. We have failed because we have not followed nature with sufficient closeness. We require the earnest endeavours of the experimental physiologist to be combined in the closest association with the most extended and searching investigations of the systematist. Either alone must of necessity be useless.

Mr. Lawrence Balls informs us that his description of "Gossypiæ" as a sub-order (instead of a tribe) had been taken from "an accepted authority on systematy." He might have favoured us with the name of the author in question. I have searched through a fairly extensive botanical library and failed to discover the authority to whom he may be alluding. Bentham and Hooker, in their "Genera Plantarum," place Gossypium in the tribe Hibisceæ (which Mr. Balls renders as Hibiscæ), but they make no mention of a sub-order "Gossypiæ" (? Gossypeæ). These are no doubt trivial criticisms and are made only in the spirit of "Hindu" and "Hindi." But admitting the "accepted authority on systematy," is there any advantage in setting on one side the universally accepted authorities on British botany?

I am afraid Mr. Lawrence Balls simply tries to obscure the main issue, raised in my review, by citing an example of careless orthography; the "Hindu" and "Hindi-weed" already mentioned. Is it necessary to explain that the word "Hindu" denotes the people or the religion, while "Hindi" and "Hindustani" indicate the languages of certain portions of India? These are their most general acceptations, but neither could, strictly speaking, be used as the name of a plant, more especially when *that* plant never could have come from India. The person who first used that name, in its Egyptian signification, was very possibly a follower of the school that seems to hold the view that accuracy in systematic botany was an unpardonable offence. De Candolle, long years ago, told us that the aim of science was not to make names, but to use names to distinguish plants. Does "Hindi-weed" isolate a certain cotton plant from all others? If it does not, it is a vulgar name that should find no place in a scientific publication.

Sir George Watt's "Wild and Cultivated Cotton Plants of the World" (to which Mr. Balls refers us) mentions Hindi-weed as being possibly a recessive hybrid of the *Moqui* of Arizona, or perhaps rather of the *N'dargua* cotton of Senegal. It is not advanced as a name that can be accepted as distinguishing a definite plant. But Mr. Balls himself is quoted by Sir George (*loc. cit.*, p. 182) as holding that Hindi-weed "hybridises with the others and the Mendelian splitting forms from the cross are very common, and also go under the name of 'Hindi,' though they are usually very tall, up to three metres. 'Hindi' itself is about one metre high, and except in its seed reminds me of American Uplands." We are thus told, by an advocate of non-systematic studies, that "Hindi-weed" may assume numerous forms and conditions until a certain example of it might have to be spoken of as not being Hindi-weed. Thus that vulgar Egyptian name is by no means as "definite as any other," though Mr. Balls in another passage assures us that it is. It is a loose, popular name that could never be taken seriously as the name of a cotton plant. The issue raised by Mr. Balls as to the Hindi-weed having a naked seed, while he seems to affirm that Sir George Watt "during the primary division of the genus" places it with fuzzy-seeded forms (a passage I have failed to discover) is, however, outside the scope of a review of Mr. Balls's book.

Lastly, I admit that Mr. Balls's jest of scientific names being merely useless duplicates of easier names was not only feeble (as he now admits it to have been) but highly misleading and utterly out of place.

THE REVIEWER.

THE KARAKORAM EXPEDITION.

THE account of Cav. Dr. F. de Filippi's expedition of 1914–15 to the eastern portion of the Karakoram range, briefly noticed in NATURE of August 5, has now been published in the *Geographical Journal* (vol. xlvi., No. 2), with a selection from the beautiful photographs taken by Capt. Antilli, to whom this part of the varied and

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